It is, indeed, so far as the present writer is aware, the only modern work which deals with the construction of large coils from a thoroughly practical standpoint. It describes in every detail the making of the apparatus, and contains much valuable information as to the general design of coils, the methods of winding and the processes of insulation, which hitherto have been the carefully preserved secrets of the very few makers of coils of the more powerful descriptions. Questions of cost are not omitted, while special chapters are devoted to contact breakers of the mercury, hand and electrolytic types.

The discovery of the Röntgen rays, and the important application that these have found in surgical and medical practice, together with the increasing employment of high tension electrical discharges in wireless telegraphy, spectroscopic analysis and other fields, have brought about a great demand for Rhumkorf coils of large size. The need for a book such as the one under review has therefore become increasingly felt of late years, and the only matter for regret is that the author did not give to the public the results of his experience at an earlier date.

The book is clearly written, well printed and well illustrated. A. A. C. S.

The Structure and Life-History of the Harlequin Fly (*Chironomus*). By L. C. Miall, F.R.S., and A. R. Hammond, F.L.S. Pp. viii + 196; plate and text illustrations. (Oxford: Clarendon Press, 1900.)

THE perfect insects of *Chironomus* are conspicuous objects on our windows, or may be seen dancing in swarms in the open air, and are often called "gnats," to which they have considerable resemblance; and, like gnats, the antennæ of the males are very plumose. The larvæ are found at the bottom of standing or slowly-running water, and those of some species are known, from their colour, as "blood-worms," while those of other species are green. The insects are easily collected and reared, and present many points of interest; and the work before us gives a very clear and fairly elaborate account of the structure and habits of these insects in their various stages. The life-histories of insects present an inexhaustible field for the investigations of any observers who care to devote their attention to this branch of entomology; and books like the present will give the beginner a very good idea of the best way to work on similar lines. Hitherto the Diptera, though one of the largest orders of insects, have been strangely neglected in England, and we have not even a good descriptive book on the order, though almost every Continental country has a good monograph in its own language. The interest felt in mosquitoes, however, at the present time will probably spread to other insects of the same order; and thus we are likely to see the study of their life-histories leading to that of the Diptera as a whole, instead of interest in the order generally, leading to researches into its life-histories, as has been the case with some of the other orders of insects.

The bulk of this work is too technical and too elaborate to admit of its being discussed in detail, and it contains much useful general information relating to allied species, nor are the parasites of the larvæ left unnoticed. One remark strikes us as specially interesting: "No insect is known to us which has more completely departed from the habits and structure of an air-breathing animal. Yet even here we find visible proof of descent from a terrestrial insect with branching air-tubes."

In an appendix we find a section on "Methods of Anatomical and Histological Investigation," and an additional note by Mr. T. H. Taylor on the swarming and buzzing of Harlequin flies. The book concludes with a good bibliography and index. W. F. K.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.]*

On the Nature of the Solar Corona, with some Suggestions for Work at the next Total Eclipse.

IN an article on the corona, published in the November number of the *Astrophysical Journal*, I suggested a method by which the existence of the Fraünhofer lines in the spectrum of the corona might be detected. The method was based on the supposition that the light emitted by the particles, in virtue of their incandescence, so overpowers the reflected sun-light that the lines are invisible. That the coronal light is strongly polarised is well known, and there is scarcely any doubt but that the polarised light is reflected sun-light. If, now, a Nicol prism be placed before the slit of the spectroscope in such a position as to transmit the polarised radiations, these will be allowed to pass with almost undiminished intensity, while the emitted or unpolarised light will be reduced in intensity by one-half. The great change in the ratio resulting might easily be sufficient to bring out the dark lines distinctly. I feel firmly convinced that this experiment should be tried at the Sumatra eclipse of next May, for I have successfully accomplished it in the laboratory with an artificial corona. It was found that a gas flame in a strong beam of sun-light shone with a pure bluish-white light, due to the reflection or rather scattering of the sun-light by the minute carbon particles.[1] The flame thus illuminated showed the Fraünhofer lines distinctly, but by reducing the intensity of the sun-light a point was reached at which they disappeared, and the spectrum appeared continuous. The light scattered by the flame was found to be completely plane polarised in certain directions, giving us just the required conditions, namely, particles emitting a continuous spectrum, and scattering a polarised solar spectrum. In front of the slit of the spectroscope a Nicol prism was arranged in such a manner that it could be drawn into and out of position by a cord. The Fraünhofer lines could be made to appear by sliding the Nicol in front of the slit, and disappear by drawing it away. While it does not by any means follow that the use of a Nicol on the actual corona will bring out the lines, the experiment seems to be well worth trying, as it would furnish further information regarding the relative intensity of the emitted and reflected light.

Another interesting point is that the minute particles in the flame do not scatter the longer waves, the flame reflecting practically no red or orange light. Thus the Fraünhofer lines can only be traced to about the D lines. By reducing the intensity of the sun-light they disappear, first in the yellow, then in the green, blue and violet in succession. This indicates that our chances of detecting the lines in the spectrum of the corona will be greatest in the photographic part of the spectrum. Moreover, it appears to explain the absence of radiant heat in the light sent to us from the corona, the particles being too small to scatter these longer waves to any appreciable extent. Abbott, of the Smithsonian party at Wadesboro', found the corona cold in comparison with his bolometer, and infers from this that the corona neither reflects sun-light nor emits light in virtue of incandescence, expressing the opinion that the luminosity is analogous to that of vacuum tubes transmitting electric discharges. It seems to me that the polarisation of the coronal light makes this theory untenable, and that the absence of heat rays can be explained fully by the small size of the particles. I am aware that the absence of radiant heat in the *emitted* light has yet to be accounted for. My own notion, based on experiments which are now in progress, is that the reflected or scattered light is vastly in excess of the emitted, and that the absence of the Fraünhofer lines is more probably due to the line-of-sight motion of the particles than to simple drowning out by emitted light.

My experiments on the ratio of emitted to scattered light of a body brought to incandescence by powerful solar radiation are not yet completed, consequently I do not yet feel prepared to make any very positive statement in regard to this matter. A

[1] Since writing the above I have found that the reflection of light by a flame has been described by Mr. Burch and Sir George Stokes indepen dently. It was noticed also by Soret at a still earlier date (1875) as I have subsequently found.

full account of this work will appear shortly in the *Astrophysical Journal*.

Any observers planning to use a Nicol prism in connection with a spectroscope in the manner described will find a gas or candle flame illuminated with a beam of sun-light, concentrated by means of a large mirror or lens, extremely useful in making preliminary experiments.

For work on the polarisation of the corona, I believe that the artificial corona, which will be described next week, will be found most useful for preparatory work. Not only is it polarised, and polarised in the same way as the real corona, but it resembles it in every respect, and can be easily made of the same brilliancy. It would be well to work with particles of different size, giving different percentages of polarisation, and the picturesque refinements for producing the polar streamers could, of course, be omitted. A lamp with a ground glass globe might be used to advantage, giving a distribution of polarisation more nearly like that of the actual corona.

Data regarding the plane of polarisation in the streamers would be useful in formulating a theory of the streamers. These, it seems to me, can be conceived as formed in two ways: they may be streams of coronal particles moving in curved paths, in which case the plane of polarisation should be everywhere strictly radial, or, what is extremely improbable, they may be caused by divergent beams of light coming from the polar regions of the sun and moving in curved paths owing to the rapid decrease in the refractive index of the sun's atmosphere in an outward direction. If this were the case, the plane of polarisation would turn with the streamer. This latter hypothesis is extremely visionary, and I do not present it seriously, for it is almost impossible to conceive of any way in which the isolated beams of light could be formed, unless, perhaps, by vortex funnels more highly luminous than the surrounding surface of the sun. Such fanciful speculations are hardly worth indulging in, though they have interested me for the moment in connection with the matter of possible curvature of light rays in the sun's atmosphere, alluded to in a recent paper by Julius in the *Astrophysical Journal*.

R. W. WOOD.

University of Wisconsin.

The Alleged Decadence of German Chemistry.

As a man of business, more or less interested in the course of chemical discovery in so far as it affects chemical products of market value, I have for so many years been accustomed to take note of the rapid strides made by Germany in the chemical industries, that the statement contained in the article by "W. J. P." in your issue of December 27 (p. 214) has struck me with amazement. The writer says that "all students of contemporary chemical literature will agree that in Germany the science of chemistry has been in rapid decadence during recent years." This statement seems to me so completely at variance with my own experience that I have consulted chemical friends as to its accuracy, and I cannot find any chemist who agrees with this verdict. The consensus of opinion is, in fact, all in the opposite direction. "W. J. P." himself admits, as a generally recognised principle, that supremacy in any particular industry goes hand in hand with supremacy in the related sciences. Every one of the discoveries recorded in his own paper has been made in Germany, and he himself points out that the new industry is "almost wholly of German origin." Of course, as an English merchant, I hold no brief for German products, but having long ago recognised the importance of the connection between science and industry, which the author emphasises, and seeing what Germany has been doing of late years, I perhaps innocently attributed the progress of that country to their superior system of training in chemistry and related sciences, and to the readiness of their manufacturers to avail themselves of the results of scientific discovery. For the sake of British industry, I shall be only too glad to learn that I was mistaken; but since no chemist of my acquaintance agrees with the writer, and since he himself puts forward a whole body of German discoveries in order to tell us that chemical science is undergoing "rapid decadence" in that country, I cannot but feel that there is such a glaring contradiction between the facts recorded and the conclusions arrived at by the writer that some further explanation as to his meaning is necessary.

S. N. C.

Secondary Sexual Characters.

MR. POCOCK (p. 157) has replied to my letter, but he has not replied to my reasoning. It is no reply to say that it may be doubted whether my hypothesis is an improvement on certain others, when no reasons are given for the doubt. It is no argument to say that a problem is insufficiently supported by evidence, and may be true or false. A problem may be solved, but it cannot be either true or false, nor can it be supported by evidence. Mr. Pocock himself in his article attributed the colour of the male nilghaie and other antelopes to "male katabolism," which he now says is nothing but an imposing substitute for the "vital force" of the pseudo-scientific realists. I quite agree with him, and only hope that in future he will not explain male peculiarities by attributing them to male katabolism.

It is very difficult to reason with a naturalist who uses the terms "struggle for existence" and "influence of external conditions" as equivalent to "selection." I quite understand that to the Darwinian the only important action of external conditions is the selective action, the survival of the fittest. But the Darwinian does not appear to understand his opponents' conception of the modifying non-selective action of external conditions. Mr. Pocock does not distinguish between variations and modification. If any cause acts on *all* the males of a species and makes their colours dark or black, what effect can selection produce? If the dark colour is harmful, the species will become reduced or extinct. But selection cannot, as Mr. Pocock suggests, "check" a general modification due to a general cause without eliminating the species.

Thus the question which Mr. Pocock raised in his original article, and which he now "sets aside," the question of the initial cause of sexual modifications, is the essential question of the whole subject, and cannot be set aside in any rational discussion of the facts. Even supposing that the variations are different and not general, and that those which are beneficial are selected and preserved, selection offers no explanation of the fact that in so many cases the peculiarities in question are inherited only by the male sex. Mr. Pocock, in discussing the uses of coloration and markings, was obliged to refer to cases in which the males differed from the females both in colour and in horns. He has not yet realised the truth that no theories based on the conception of selection afford any explanation of unisexual inheritance.

Penzance, December 17, 1900. J. T. CUNNINGHAM.

The Word Physiography.

WITH reference to the question of the early use of the word Physiography to express the comprehensive study of Nature, to which you refer on p. 207 of your last issue, I should like to call attention to a fact which appears to have been almost forgotten.

The title-page of a "Dictionnaire des Termes usités dans les Sciences naturelles," published in Paris in 1834, bears as a motto the words—

"Profectò physiographiam qui colit, ullo pacto metam perfectioris cognitionis feliciùs non attinget, quàm si aliquot dies terminis perdiscendis tribuerit."—*Linné*.

I have tried, but without success, to find this quotation in the works of Linnæus; perhaps some of your readers may be able to supply the reference. The word Physiography was certainly current in Sweden about the middle of the eighteenth century, as in the obituary notice of Torbern Bergman, read at the Stockholm Academy of Sciences in 1786 (which I know only in the German translation), it is mentioned that he became a member "der Physiographischen Gesellschaft in Lund, 1776." "Minerva" for 1900—1901, however, states that the Physiographical Society of Lund, which still exists, was founded in 1778 for the study of the scientific and economic conditions of the province of Scania. There was at the same time a Cosmographic Society in Upsala, and the two names seem to have been used much in the same sense.

I think it possible that the word Physiography was introduced in Sweden by Linnæus as a substitute for Cosmography, the ancestor alike of the Physiography of South Kensington, and the Physical Geography of the older text-books.

HUGH ROBERT MILL.